that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. . . The results of our acts become the first objects of our conceptual thought." The truth of these statements I venture to question. After noting the dogmatic nature of the assertion "Nothing but this *could*, &c.," I must object to the statement of fact as regards human beings now. I do not believe that the infant's first object of thought is "the results of its own acts." In the first place, no object of our early thoughts is merely the "results of our own acts," but a combined result of our own activity and of the action on us of our environment. Secondly, my observations lead me to believe that the infant's first thoughts relate to things external, and certainly not to the results of its own activity as such, which is a highly complex and developed thought. It may be that the Professor, when he says "The results of our acts *become* the first object of our conceptual thoughts," means that such acts in remote antiquity *became* the object of man's first thought. This is probably the case, since, with respect to the origin of thought and language, Prof. Max Müller has adopted Noiré's crude notion that they sprang from sounds emitted by men at work, conscious of what they were doing, in the presence of others who beheld their actions and heard the sounds; the result being the formation of a conceptual word, to attain which five stages had to be gone through, as follows:—

"(1) Consciousness of our own repeated acts.

"(2) *Clamor concomitans* of these acts.

"(3) Consciousness of our *clamor* as concomitant to the act.

"(4) Repetition of that *clamor* to recall the act.

"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c."

But, if language and reason are identical, reason could not exist before a single conceptual word existed. Nevertheless, to attain to this first single word, we see, from the above quotation, that man must have had the notion of his own acts as such; the notion of their repetition; the notions of clamor, action, and the simultaneity of clamor and action; the will to recall the act (yet *nihil volitum quia præcognitum*); and finally the notions of consequence, instrumentality, agency, or whatever further notions the Professor may intend by his "&c."

Thus he who first developed language must be admitted to have already had a mind well stored with intellectual notions! But can it for one instant be seriously maintained, close as is the connection of language with reason, that their genesis (miracle apart, of which there is no question) was *absolutely* simultaneous? He must be a bold, not to say a rash, man who would dogmatically affirm this. But if they were not *absolutely* simultaneous, one must have existed, for however brief a space, before the other. That intellectual language could have existed without reason is absurd. Reason, then, must, for however short a period, have preceded language.

In conclusion, I desire to point out a certain misrepresentation with respect to natural selection. The Professor says: "In the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest." But, we may ask in passing, if reason has no existence, how can it "triumph"? The misrepresentation of natural selection, however, lies in his use of the word "fittest." When biologists say that the "fittest" survives, they do not mean to say that that survives which is the most "reasonable and right," but that that survives which *is able to survive*. What there is less "reasonable and right" in a Rhytina than in a Dugong, or in a Dinornis than an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason, in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits. ST. GEORGE MIVART.

Mechanical Equivalent of Heat.

I FIND that the mode of regarding J advocated in my letter in last week's NATURE (p. 320) is not quite new, for my brother, Dr. Oliver Lodge, writes to tell me that Clerk-Maxwell, on p. 298 of his "Theory of Heat," has called J the specific heat of water. However, he has not done so throughout the book, and I do not think it is the meaning generally attached to the symbol, though it seems to me that it should be so; that is to say, J should always be considered as denoting the specific heat of water at the temperature 0° C. ALFRED LODGE.

Coopers Hill, Staines, February 6.

"Is Hail so formed?"

I CANNOT accept Dr. Rae's explanation as a "simpler solution" of the phenomenon described by me in NATURE of January 26 (p. 295), because it is based upon meteorological conditions that were at the time non-existent.

My own observation of the pine-tree convinced me that at or near the summit there was no adherent ice or rime; and had there been beads of ice upon the leaves I should still have failed to see what should have caused them while frozen to become detached and change from beads to pellets.

There was a fine mist during the whole of the day, and I observed the phenomenon at 3.30 p.m.

A letter appeared in NATURE upon the same day as mine, drawing attention to the unusual atmospheric conditions observed about that time, and containing facts which manifestly support my theory. CECIL CARUS-WILSON.

Bournemouth, February 11.

The New Army Regulations.

THE new regulations for the Woolwich entrance examination have been very unfavourably received by men of science. This hostile criticism is in some respects the consequence of the absence of clear discrimination between them and those already in force for the Sandhurst examination.

It must be remembered that candidates for Woolwich cadetships must be between the ages of 16 and 18; that 6000 marks are awarded for mathematics, with 1500 more for drawing and English composition; and that in both the last June and December competitions less than 4000 marks sufficed to place a student among the successful competitors. Since candidates can pass in these subjects alone, it appears unreasonable to complain that youths of scientific power are excluded from the Royal Military Academy. Classics are sufficiently discouraged by the fact that they have no mark value after the cadet has entered the Academy. The 5000 marks offered in the entrance examination for Latin and Greek merely serve to encourage candidates who have been educated on the classical sides, which are almost always the stronger at our public schools. They really tend to widen rather than to narrow the sources from which candidates are drawn.

After a quarter of a century of continuous experience as a student and teacher of elementary science, I find myself reluctantly forced to the conclusion that chemistry, physics, and geology are not good educational subjects for lads under 16 years of age. I believe that it is in most cases desirable that youths intended for a scientific career should not specialize too early. A sound foundation of mathematics and modern languages is almost necessary to enable them to attack their scientific subjects efficiently. With minds trained to the use of the exact and powerful processes of mathematical reasoning, and able to readily appreciate and avail themselves of the wealth of scientific literature in France and Germany, they will probably become more useful officers than if they had acquired a smattering of science.

On the other hand, your wise censure of the discouragment of science in the Sandhurst regulations must commend itself to all thoughtful men. The case is even stronger than at first sight appears in the studious moderation of your judicious article. The limits of age are higher for Sandhurst, being 20, or in some cases 24. The training of the Line cadets is less complete. As they only spend one year at Sandhurst, they are obliged to confine their attention more strictly to professional subjects. Officers of the Line have often more leisure than those in the scientific corps, and there are many reasons why even a slight acquaintance with science would be helpful to them. It also seems hard that a candidate should be handicapped by not taking up Latin. Sometimes it has been discontinued for a considerable period, and a candidate can ill afford to take up "a 2000 subject," considering the severity of the competition.

I would wish respectfully to suggest that a memorial should be presented to the War Office by all interested in the teaching of science, praying that, if a candidate for an army examination

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wishes to substitute for Latin one of the sciences enumerated in Group II., it should be allowed a maximum of 3000 marks.

2 Powis Square, W. HENRY PALIN GURNEY.

"British and Irish Salmonidæ."

As your reviewer allows that he "intentionally omitted" five words from a sentence of mine which he quoted in order to criticise, I may well leave comments on such a proceeding to your readers. I willingly acquit him of having purposely made me to suggest utter nonsense, as I cannot help thinking that his knowledge of fish-culture was such that he was unaware he was doing so.

As to the second point he says, "I doubted and still doubt if there is any method practised in which layers of moss are used and are separated from the eggs by muslin and similar material." As he rejects the Howietoun account which I gave, I now submit extracts from two standard works, one American, the other English, which will, I believe, be conclusive to those who are ignorant of fish-culture, for every fish-culturist is aware that this plan is commonly adopted. Livingstone-Stone ("Domesticated Trout," ed. 3, 1877) remarked :—"Theodore Lyman recommends placing each layer of eggs in a fold of mosquito netting to keep them from mixing with the moss and so facilitate the unpacking of them. *This is a great improvement. By all means use mosquito netting*" (p. 149). Mr. Andrews, of Guildford, wrote thus in the Badminton Series ("Salmon and Trout," 1885) :—"The plan of packing does not vary much with trout breeders. The eggs are placed in alternate layers between moss, and protected by a covering of mosquito netting, muslin, swans' down, calico, or butter cloth, so arranged that the eggs shall not be crushed or escape" (p. 447).

As regards the third point, your reviewer now appears to be convinced that *Salmo namaycush* is a char, as I stated it to be. It must be a matter of regret that he omitted to investigate the foregoing questions prior to authoritatively writing upon them in such a well-known publication as NATURE.

Cheltenham, February 4. FRANCIS DAY.

IN his last letter Mr. Day has certainly proved the correctness of the statement in his book that salmonoid eggs are packed with layers of moss from which they are separated by muslin or other textile fabric. If I had known as much about salmon-culture as he, I certainly should not have questioned the statement; it is to be noted that I only questioned and did not deny. If I had been as completely versed in the knowledge of Salmonidæ as Mr. Day, I should have written a book on the subject instead of reviewing his. But the essential point, which Mr. Day seems incapable of appreciating, is this : that there was nothing in the notes on the subject of packing in his book which confirmed the statement in the text ; and although my doubts as to the correctness of that statement are removed by his letter, they were perfectly justifiable in a reader of his book. Mr. Day does not apparently suspect that people interested in the subject, including the reviewer, read his book for the sake of gaining information, and not because they already know as much about the subject as himself. All I had to do was to give my impressions of the book as I found it : the fitness of my criticisms is only the more established by the lengthening appendix to his book which Mr. Day is now publishing in your correspondence columns. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. MAGNETISM—(*continued.*)

VIII.

IT will now be perceived that a fly-wheel in rotation is the mechanical analogue of magnetism, or more definitely of a section of a line (or tube) of magnetic force ; and that a brake applied to such a fly-wheel, with consequent slip, dissipation of energy, and production of heat, is in some sort a mechanical analogue of an electric current.

The field is regarded as full of geared elastic vortices or whirls, some of which are cogged together, so to speak, while others are merely pressed together by smooth rims. It is among these latter that slip is possible, and in the regions occupied by them that currents exist ; the energy dissipated here being transmitted through the non-slippery or dielectric regions from the source of power, just as energy is transmitted from a steam-engine through mill-work or shafting to the various places where it is dissipated by friction.

Mechanical Force acting on a Conductor conveying a Current.

In Fig. 41 the conducting portion is shown with opposite rotations on either side of it. Now superpose a uniform rotation all in one direction upon this, so as to increase the spin on one side and diminish it on the other. Immediately the extra centrifugal force on one side will urge any movable part of the conductor from the stronger to the weaker portion of the field.

The field for a direct and return circuit may be similarly drawn by superposition of their separate whirls (see Fig. 40) ; and so it becomes evident why a circuit tends to expand so as to inclose the largest possible area, even if no other magnetic field than its own be acting on it.

Also if two circuits are arranged near each other in a plane, with their currents in opposite directions, they will more or less neutralize each other's effect on the space between them, causing (if equal) a region of no spin there. Their neighbouring portions will thus get urged together by the unbalanced pressure on the other side : or, currents in the same direction attract.

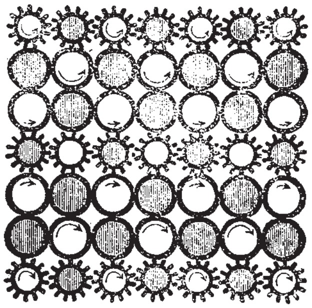

FIG. 44.—Two parallel conductors conveying equal currents in one direction and getting pushed together by the centrifugal force of the outside whirls, no whirl existing between them. The length of the arrows again suggests the distribution of magnetism in the conductors. Fig. 40 showed the correlative repulsion of opposite currents.

As for the effect of iron introduced into a circuit, it brings into the region of space it occupies some two or three hundred times as many lines of whirl as were there before, and these naturally contribute mightily to the effects, both those exhibiting mechanical force and those exhibiting inertia.

When one says, as roughly one may do, that iron brings 300 fresh lines into the field, one means that for every whirl otherwise excited, 300 more are faced round in the iron. And this process goes on while the field is increasing in strength until the total number of whirls in the iron begins to be called upon ; when this point is reached the rate of addition is not maintained, and the iron is said to show signs of saturation. Ultimately, if ever all its whirls were faced round, the iron would be quite saturated ; but long before this point is reached another cause is likely to make itself felt, viz. the falling off in the strength of the whirls already faced round, by the action of the strong magnetic induction, which is all the time acting so as to weaken the iron currents so far as it is able. And thus at a certain point hitherto unreached by experiment the iron may not only fail to increase the strength of the field any more, but may actually begin to diminish it.

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[1] Continued from p. 348.

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